upon the surface of the moving mass, and not secondarily through the intervention of air-currents, electricity, or evaporation and condensation."

In a paper subsequently communicated to the Royal Society (*Proceedings*, March 12, 1875), Mr. Crookes characterised the explanation of the "repulsion from radiation" offered by Prof. Osborne Reynolds, as one which "*it is impossible to conceive*," the phenomenon taking place in a *chemical* vacuum. At the same time he stated that he was unprepared to offer any other explanation, and that "he should avoid giving any theory on the subject until a sufficient number of facts have been accumulated."

After bringing out the Radiometer, however, he reverted (as it seemed to me) to his previous "impression;" the whole phraseology of his papers of January 5 and February 5, 1876, appearing at the time, not only to myself, but to every one of the eminent scientific friends with whom I conversed on the subject, to indicate that he *then* considered the rotation as *directly due to the impact of the waves upon the surface of the moving mass*. Nor have I ever imputed it to him as a matter of blame that he took this view of it; on the other hand I have stated over and over again that this seemed the general impression of the distinguished Physicists to whom we "outsiders" looked for guidance in the matter. Anyone who remembers what took place at the Meeting of the Royal Society at which Mr. Crookes's paper was read, will, I feel sure, bear out this statement.

I shall now specify more explicitly the grounds on which I attributed to Mr. Crookes, no longer as an "impression," but as a definite "interpretation" of his facts, that the rotation of the Radiometer is due to the *direct impact of the waves*, and chiefly (I never said exclusively) to those of the *luminous* waves; and further attributed to him a claim to the discovery of a "new force" or "new mode of force."

This *key-note* seems to me to be most distinctly struck in the following passage:—After pointing out that "there is no real difference between Heat and Light, all we can take account of [I presume he means physically, not physiologically] being difference of wave-length," he thus continues: "Take, for instance, a ray of definite refrangibility in the red. Falling on a Thermometer it shows the action of *Heat;* on a Thermopile it produces an *electric current;*[1] to the Eye it appears as *light and colour;* on a Photographic plate it causes *chemical action;* and on the suspended pith *it causes motion*."

Now (1) this motion being elsewhere spoken of as due to *the impetus given by a ray of light*, (2) a set of experiments being made to determine *the mechanical values of the different colours of the spectrum*, (3) an observation being recorded on *the weight of sunlight* (without the least intimation that he was "speaking figuratively," as Mr. Crookes says that he did to his audience at the Royal Institution), (4) the term *Light-mill*[2] being used by himself as a synonym for "Radiometer," and (5) no hint whatever being given of the dependence of the result (as argued by Prof. Osborne Reynolds) on a "heat-reaction" through the residual vapour, I still hold myself fully justified in attributing to Mr. Crookes the doctrine of *the direct mechanical action of light;* and I call on Prof. Carey Foster to prove—not that Mr. Crookes himself did not hold that doctrine—but (which is a very different thing) that I am not justified by Mr. Crookes's own language in attributing it to him.

That Mr. Crookes considered such action a "new force" or a "new mode of force," plainly appears from my previous citation; in which he ranks Motion as a mode of Radiant action *additional* to Light, Heat, and Actinism, differing as much from either of them as they differ from each other. If it does *not* mean this, what does it mean?

So, if Mr. Crookes has *not* changed his mind as to the interpretation of his facts, I ask (1) why he now repudiates as inappropriate the term *Light-mill* adopted (if not originally given) by himself? and (2) why does he now admit that dependence of the movements upon the presence of residual gas, which he originally affirmed to be *impossible to conceive?*

I have carefully confined myself to the *main issues* of this question. Prof. G. Carey Foster will doubtless be able to pick out *points of detail* in my article, as to which fault may be found by a severe critic. But I venture to think that I have said enough to prove that what I said on the subject was written under the honest conviction that I had adequate ground for my statements; and that I shall at any rate be absolved from the imputation of having ill-naturedly referred to the history of the Radiometer for the purpose of putting Mr. Crookes in the wrong; the "lesson" with which I concluded the article being as follows:—

"The lesson which this curious contrast [the 'duality' of Mr. Crookes's mental constitution, which I speak of as having plenty of parallels in past times, to say nothing of the present] seems to me most strongly to enforce, is that of the importance of training and disciplining the *whole* mind during the period of its development, of cultivating scientific habits of thought (by which I mean nothing more than strict reasoning based on exact observation) in regard to *every* subject, and of not allowing ourselves to become 'possessed' by any ideas or class of ideas, that the common sense of educated mankind pronounces to be irrational. I would not for a moment uphold that test as an infallible one; but it ought to be sufficiently regarded, to make us question the conclusions which depend solely upon our own or others' subjectivity, and to withhold us from affirming the existence of new agencies in Nature, until she has been questioned in every conceivable way, and every other possibility has been exhausted." (*Op. cit.*, p. 256.)

October 10 WILLIAM B. CARPENTER

I HEAR from Dr. Carpenter that he is sending to you, for publication in the next number of NATURE, a statement intended as a refutation of an opinion which I ventured to express, in my address to the Mathematical and Physical Section of the British Association at Plymouth, respecting an article on the "Radiometer," contributed by him to the *Nineteenth Century* for April. As Dr. Carpenter appears to have interpreted that expression of opinion in a sense different from that which it was intended to convey, I shall be much obliged if you will afford me space in your next issue for a few words of explanation.

The words which I used in referring to his article were these: "An eminent and accomplished scientific man had published, within the last few months, an account of the discovery of the radiometer, the unmistakable tendency of which was, either intentionally or unintentionally, to depreciate Mr. Crookes's merits, and to make it appear that he had put a wrong interpretation upon his own results." The word *depreciate*, which occurs here, is, I am aware, susceptible of various shades of meaning, and perhaps it would have been better if I had guarded myself against the possibility of misconception that lurks in it. What I meant was that Dr. Carpenter's account of Mr. Crookes's researches was likely to make his readers form a lower estimate of their scientific value than, in my opinion, they deserved; but whether or not it was *intended* to have this effect I did not undertake to say. I did not mean, and had not the smallest idea of suggesting, that Dr. Carpenter had been guilty of intentional and conscious unfairness towards Mr. Crookes. I should have thought it entirely unnecessary to disavow this latter interpretation of my words, and indeed should have considered it an insult to Dr. Carpenter's reputation to suppose that anyone would understand them in this sense, had he not himself (in a recent correspondence with me) endeavoured to fix this meaning on them in spite of my repeated assurances that it was not intended.

I do not wish to say anything on this occasion in support of the opinion which I have admitted that I did intend to express, but I shall ask you to allow me to do so in a future number of NATURE. G. CAREY FOSTER

University College, London, October 14

Mr. Wallace and Reichenbach's Odyle

As Mr. Wallace has attempted (presumably with Mr. Crookes's editorial concurrence) to rehabilitate, in the July number of the *Quarterly Journal of Science*, the Odyle-doctrine of Baron Reichenbach, I think it well to state that I yesterday availed myself of an opportunity of personally asking my friend

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[1] Having never heard of any physical philosopher from Seebeck to Sir William Thomson, who looked at the *electric current* generated in the Thermopile as anything else than an effect of the *heating* (whether by *conduction* or by *radiation*) of the two metals of which it was composed, I was greatly surprised at finding it ranked by Mr. Crookes as one of the *immediate* modes of Radiant action; and I called attention in my "Radiometer" paper to what I supposed to be his mistake on this point. It may be that in my ignorance of the newest developments of thermo-electric theory (my knowledge of it not being later than 1872, "Everett's translation of Deschanel," p. 652), I have here unintentionally "depreciated Mr. Crookes's merits;" and I shall be quite ready to recant and apologise for my mistake, if Prof. G. C. Foster will show that it is Dr. Carpenter, not Mr. Crookes, who is here in the wrong.

[2] It is impossible not to see, in the use of this term, a suggestion that the vanes are driven round by the direct mechanical impetus of Light upon them, in the same way as the sails of a Wind-mill are driven round by the direct impetus of the Wind.

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Prof. Hoffmann, of Berlin, whether that doctrine any longer finds support among scientific men in Germany. *His reply was a most emphatic negative;* the doctrine, he said, being one which no man of science with whom he is acquainted would think worthy of the slightest attention. Yet in Mr. Wallace's judgment (*query* in Mr. Crookes's also?) the unanimous verdict of the scientific world of Germany, to say nothing of England, is a prejudiced one; only Mr. W. and his spiritualistic allies appreciating correctly the real force of the evidence originally advanced by Reichenbach, and confirmed by those trustworthy (?) authorities, Drs. Ashburner and Gregory.

In thus setting his own judgment on a question which lies altogether outside the scientific domain which he has made his own, against the unanimous verdict of the eminent physicists and physiologists who have carefully "tried" the Od-force and "found it wanting," and in rebuking myself and those who think with me for our incredulity, does not Mr. Wallace put himself somewhat in the attitude of his old opponent, John Hampden, who thinks everybody either a fool or a knave who maintains the earth to be round? WILLIAM B. CARPENTER

October 22

Potential Energy

WITH reference to the views of "John O'Toole" on the subject of energy perhaps you will allow me to say how one of the class to which "poor Publius" belongs has conceived the matter of terminology with satisfaction to himself.

1. Energy being unanimously defined by "the doctors" to be "capacity for doing work," and also energy conveying in its derivation the notion of activity, this term is properly applicable only to the bodies of material systems the motions of which are contemplated. Hence all energy is in its nature kinetic—the very term kinetic is logically included in the term energy.

2. When a material system is in motion it actually possesses, *ipso facto*, a capability of doing work, that is to say, it has actual energy.

3. When in any configuration of the system we contemplate as possible the action of causes which will alter its motions and give it a second configuration, the excess of the energy which it would possess in this second configuration over the energy which it possesses in the first is properly called its potential energy in the first configuration.

4. The assertion that in any configuration the sum of the energies, actual and potential, of a material system is constant, is what Kant would call an analytical proposition, or what "X." (quoting Herschel) calls "only a truism after all." But I further remark—

5. That this truism is not the principle of the conservation of energy, but that this principle is a true "synthetical proposition" which some fairly regard as an almost immediate deduction from Newton's third law, and which others regard as proved by often repeated and much varied experiment; and hence that "X.'s" statement of this great principle in the form—"The sum of the actual and potential energies *of the universe* is a constant quantity," (the italics are mine) is not its proper definition.

6. That, leaving the consideration of bodies, and referring to forces, the term to be employed instead of energy is work, and that the term analogous to the "potential energy of bodies" is the "potential work of forces," this latter being the amount of work which they are capable of doing in displacing their points of application from their actual configuration to any fixed chosen one.

7. That by the expenditure of a fixed amount of work on any material system the same amount of actual energy (whose type is $\frac{1}{2}mv^2$) is under all circumstances produced, and that, through whatever forms this actual energy is made to pass, if the whole of it is always utilised, it will finally be reconvertible into the same original amount of work, this being the principle of the conservation of energy.

8. That instead of the statement in 5, we must substitute the synthetical proposition that "the sum of the actual energy of the bodies in the universe and the *potential work of its internal forces* is a constant quantity," and the same is true of every material system which is regarded as complete in itself; or in other words, wherever and however a given quantity of potential work is lost by the forces of the system, this always appears in the shape of a fixed quantity of actual energy, in the form which we call heat, or in some other.

Hence we have energy, actual and potential, of bodies; and work, actual and potential, of forces.

A few remarks in conclusion. "J. M." has very happily illustrated the propriety of the expression potential energy, as, in strict consequence of the definition of energy, a potential capacity of doing work; and if in his illustration the "power of purchasing" is considered with reference to a further object, there may be not merely a "double remotion from" what we may regard as "tangibility," but a remotion of a higher multiple order. "W. G." has well explained that it is only in consequence of the fixedness of the earth that the potential energy of the system of the earth and stone is by the "doctors" located in the stone. Finally, I can hardly conceive how "X.," who has devoted so much attention to the literature of this subject, can have fallen into such a grievous error with regard to the clock.

Royal Indian Engineering College, G. M. MINCHIN
Cooper's Hill

YOUR "Potential Energy" corespondents will find three letters on the "Conservation of Energy" in the *Engineer* for January 12 and 19 and February 2 which may interest them. The writer "ΦΠ" assumes that all the phenomena of force are explained by the theory that only matter and motion exist, and that what we call potential "energy" is only "quantity and motion," which motion is indestructible but diffusible. Z.

London, October 20

Origin of Contagious Diseases

I HAVE been much struck by the following passage in Dr. Richardson's address, NATURE (vol. xvi. p. 481):—

"(*c*) That as regards the organic poisons themselves and their physical properties, the great type of them all is represented by the poison of any venomous snake. . . . It is the type of all the poisons which produce disease."

Now has it been really proved, by experiment, that the poison of snakes produces the effects characterising the contagia? viz.,

"(*o*) . . . Each particle of any of these poisons brought into contact either with the blood of the living animal or with certain secretions of the living animal, possesses the property of turning the albuminous part of that same blood or that same secretion into substance like itself. . . ."

In other words, if an animal is suffering from snake poison does its blood or any of its secretions acquire the power of transmitting the disease, *i.e.*, the effects of a snake's bite, to another individual, as is the case with an animal affected with carbuncle, glanders, hydrophobia, &c., &c.?

Unless this question has been decided in the affirmative it would appear rather difficult to uphold the sentence (*c*) as quoted above. D. W.

Freiburg in Brisgau, G. J., October 14

[Dr. Richardson informs us that D. W. does not properly understand his argument. Dr. Richardson does not suppose that the person or animal poisoned from a poisonous snake is, in turn, poisonous, although that may be the fact. He merely uses the illustration that as a poisonous snake secretes a poison so an infectious person is for the time secreting a poison.]

I SEE by your issue of October 4, that Dr. Richardson has honoured me by mentioning my name and placing me as the first, in modern times, to advocate the hypothesis that living germs are the exciting agents of epidemic and infectious diseases. But he says further, "I protest, I say, that this hypothesis is the wildest, the most innocent, the most distant from the phenomena it attempts to explain, that ever entered the mind of man to conceive." It may be so, but I look in vain through the whole story he narrates in his lecture to find a rational substitute for it, and it appears to me desirable at the present juncture that the principles of the germ theory, as I have interpreted them, should stand side by side with Dr. Richardson's "glandular theory." It is now nearly thirty years since I endeavoured to find some common root or cause for those diseases which we find in plants, animals, and man, and which are communicable among the individuals of each order in nature; also, in some instances, from one order to another. During that thirty years every step in scientific research and medical experience as far as my inquiries have carrried me, has tended to confirm the views I put forward in my original "Essay" and in subsequent papers read before the Epidemiological Society. Notably the latest advocates of a germ theory are two of our most eminent men, the one a leader in science, the other a leading physician. I need hardly say I allude to Prof. Tyndall and Sir Thomas Watson; surely these

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